A LETTER

TO

Dr *CHEYNE*;

OCCASIONED BY

Dr *Robinson*'s Letter to Him,

In Defence of his Treatise of the

ANIMAL OECONOMY,

A G A I N S T
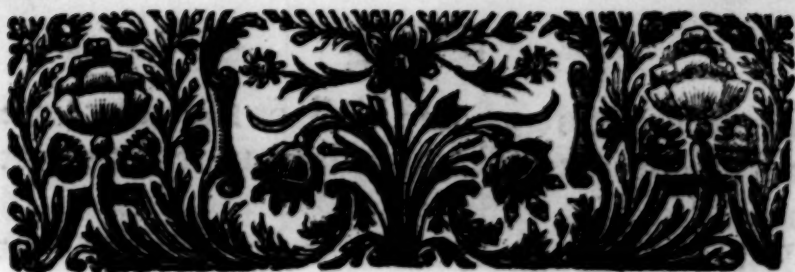
Dr *Morgan*'s Objections

IN HIS

MECHANICAL PRACTICE, &c.

By T. MORGAN, M.D.

A
LETTER
TO
Dr *CHEYNE*.

SIR,

AS Dr *Bryan Robinson* has appealed to you in the controversy between him and me, occasioned by his treatise of the *Animal Oeconomy*; I accept the appeal, and make you the arbitrator in the cause. Though we scarce agree in any thing else, yet we both agree in this, that you are a very proper and competent judge of the matter in debate. And therefore, Sir, I hope, that, even for the sake of the public, you will seasonably interpose, and not suffer us *Mathematicians* to dispute and wrangle like *Metaphysicians* and *School Divines*; which might tempt people to imagine, that there is no more in one than in the other; and that all preten-

A 2   sions

fions to literature of one kind or the other, are nothing elfe but different ways and tricks to get money.

It is now, I think, above two years ago, that Dr *Robinfon*'s letter to you in defence of his book came to my hands. For as it was printed in *Dublin*, and could not be had at any of the fhops in *London*, it was a long time, feveral months, before my Bookfeller could get it from *Ireland*. When I firft faw it, the anfwer to me in defence of himfelf appeared fo extreamly weak and ill-grounded, that I did not think fit to give you any trouble about it. But as he has now publifhed a third edition, with very large additions, of his work, printed in *London*, I have thought it neceffary and high time to make this appeal to you. In which, Dr *Robinfon* and I agreeing to make you the umpire, you will do the public but a neceffary piece of juftice to arbitrate the matter between us; and as, from your natural integrity and honour I cannot doubt of this, I fhall now proceed to lay the cafe fairly before you.

The cafe is in fhort this; Dr *Robinfon* in his treatife of the *Animal Oeconomy*, fuppofes water to flow out of a cylindrical veffel through an orifice, or pipe of a given length, opened horizontally in the fide of the veffel, at a given diftance below the top furface of the water. Here the perpendicular heighth of the water above the orifice or point of efflux, he calls H : the area of the containing cylinder, A : the
fection

section or area of the orifice or horizontal pipe,
a : the length of the pipe, L : the velocity of
the water at the point of efflux is denominated
V : and the diameter of the orifice at the point of
efflux is D. From thefe notations he undertakes
to prove, that $\dfrac{A^2 H}{A^2 - a^2}$ or $\dfrac{D H}{L}$ = V. This I muft
own at firft fight appear'd to me a great paradox,
in what I obferved upon it in my *Mechanical
Practice* of Phyfic, I infifted on it, that the quan-
tities A, a, D, and L ; could not alter the ftate
of the queftion with refpect to the velocity V, at
the point of efflux, whether the water flowed
immediately through the orifice, or through a
pipe of a given length L. This, Sir, is,
in fhort, the ftate of the argument between us,
and what has been hitherto faid on both
fides you have doubtlefs feen. But Dr *Robinfon*
in his letter to you, before he comes to his an-
fwer to me, lays down more diftinctly and par-
ticularly than he had done at firft, the principles
upon which he had proceeded, and *which I had
grofsly miftaken for want of confidering more
maturely what Sir* Ifaac Newton *and he had
offered about it*. I do not envy him the honour
of ranking himfelf with Sir *Ifaac Newton*, but
whether he or I have moft miftaken Sir *Ifaac
Newton*, I muft appeal to you, Sir.

Now

Now here the Doctor's construction is this :
" If ACDB be a
" cylindrical vessel
" fill'd with water,
" AB its upper ori-
" fice [surface], EF
" a circular hole in
" the middle of the
" bottom, GH the
" axis of the cylinder
" perpendicular to the
" horizon, GI the
" axis produced till
" IH becomes equal
" to the space through which a heavy body
" must descend, in *vacuo*, to acquire a velocity
" equal to the velocity of the water in the
" surface AB; I put A for the surface of the
" water AB, *a* for the area of the hole EF, H
" for GH the perpendicular height of the water
" in the vessel above the hole; V for the velocity
" of the water flowing through the hole, and *v*
" for the velocity of the water in the surface AB;
" and lastly, if the vessel be supposed to be
" kept constantly full, by being supply'd at the
" top as fast as the water runs out through the
" hole, and the water descends from the top of
" the vessel through the hole freely, and with-
" out resistance; I say that V will be equal to the
" velocity
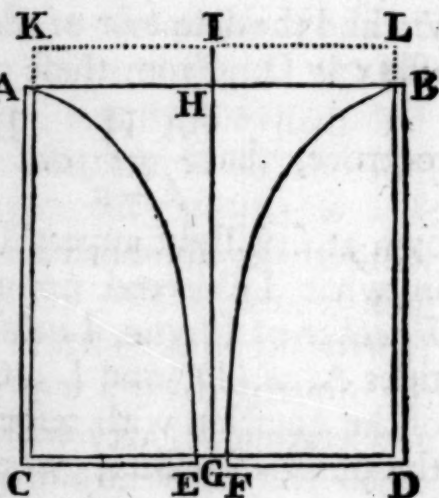
" velocity acquired in falling in *vacuo* through
" the space $\dfrac{V^2 H}{V^2 - v^2}$, or $\dfrac{A^2 H}{A^2 - a^2}$, or $\dfrac{I\,G + H}{I\,G - I\,H}$, or
" I G."

I shall not here attend minutely to the
Doctor's demonstration of this; and I believe,
Sir, when you have considered it, you would
willingly excuse both me and yourself from any
such trouble. You will readily observe, that the
Doctor supposes a certain assignable distance above
the uppermost surface of the water, to acquire the
accelerating velocity which it has at that surface,
where the accelerating motion first begins. But
can any thing be more absurd, than to go about
to determine the velocity of a fluid acquired by
accelerating gravity, at the very point where that
velocity begins? It is very plain, that at the
point H, which is the top surface of the water,
there can be no velocity by accelerating gravity,
but while a particle is considered there, it must
be considered as at rest. For till a particle at
H, has moved some space or other from H to-
wards G, it can have acquired no velocity. And
this accelerating velocity thus acquired by fall-
ing, for the first moment, or infinitely small
portion of time, will be but infinitely small, and
may be rejected as nothing, till it has acquired
some certain assignable quantity ; and then that
quantity must be taken below, and not above
H. To suppose a motion by accelerating gravity,
to begin above the top surface of the descend-
ing

ing water, is a most evident contradiction in the
very nature and reason of the thing. And yet
upon this strange hypothesis Dr *Robinson* has
writ a whole book, founded a New Theory of
Fluidity, and appealed to you, Sir, for the truth
of it.

But if the quantity I H, by the law of accele-
rating gravity, can be nothing at all, as has been
shewn, then the point I must always coincide
with H, and I G will for ever = H G. And
from hence the Doctor's fine-spun scheme of the
velocities of fluids, arising from accelerating gra-
vity, must vanish at once; the velocity of the
water falling and flowing freely, and without
resistance through the orifice, must eternally be
equal to the accelerating velocity acquired by the
fall of a column of water, or any quantity of
matter more or less, solid or fluid, through the
space H G, or to the perpendicular height of
the water in the cylinder. Now as the quantity
of water falling, whether it be only a very small
column, or the whole cylinder of water at once,
as this, I say, can make no difference in the ac-
celerating velocity acquired in falling from H to
G ; let us suppose the cylinder being filled with
water, that its bottom be immediately struck
cut, and the whole cylindrical column be left
to descend freely, and by its own gravity to G,
and in this case the velocity of the water at G
would be still the same, only a greater quantity
would be discharged in the same time ; *i. e.* on
this supposition the whole cylinder A B D C
would

would be difcharged in the fame time in which
a column fuperincumbent over the whole orifice
would have been gtherwife difcharged.

But we may fuppofe any cylindrical pipe per-
pendicular to the horizon always to be kept
full of water, as faft as it is emptied, to com-
municate with another horizontal pipe of the
fame diameter, and then the velocity at G, ac-
quired in falling through the perpendicular
height H G would be ftill the fame. And in
this cafe, which is as eafy and natural a fuppo-
fition as any other, A, according to Dr *Robin-
fon*'s notation, would be equal to *a*, and

$A^2 - a^2 = 0.$ And confequently, V or $\dfrac{A^2 H}{A^2 - a^2}$,

the velocity of the fluid at the point G muft be
infinitely great, though A and H, the area and
height of the perpendicular cylinder, fhould be
infinitely fmall. But farther it is certain, that
water, whether flowing perpendicularly or
horizontally, while it communicates in veffels
or pipes, may flow out of a narrower fection
into a wider, as well as out of a wider into
a narrower. Let us fuppofe then the dia-
meter or area of the horizontal veffel or pipe
through which the water muft flow as foon as
it is turned out of its perpendicular direction, to
be greater than that of the perpendicular veffel
or pipe, and then $A^2 - a^2$ in the Doctor's nota-
tion will be negative; and confequently the
water in the perpendicular veffel, or pipe, cannot

B   flow

flow at all downward in the direction H G, but must flow upwards in the direction H I. Thefe abfurdities and falfe confequences I had prefs'd upon the Doctor before, as the neceffary refult of his own principles and fuppofitions, and what anfwers he has given to them, and to feveral others which he ftands accountable for, you fhall foon fee.

But before I come to that, I fhall confider a little the corollaries which he here draws from his foregoing hypothefis. He fuppofes here, you fee, that the velocity at G, acquired by the accelerating gravity of the water, is not the velocity which would be acquired in falling from the perpendicular height of the fluid H G, but may be any other velocity more or lefs. And from this fuppofition he draws the following confequences.

" Cor. 1. H, the perpendicular height of the " water in the veffel, is $=$ to I G $\times \dfrac{V^2 - v^2}{V^2} =$

I G $\times \dfrac{A^2 - a^2}{A^2}$." Now if I H, the fpace thro' which he fuppofes the accelerating velocity of the fluid to begin, above the uppermoft furface of the water in the veffel, be really nothing, and merely imaginary, as I have proved; It muft prove only that H is equal to itfelf, or that it may be equal to any other quantity more or lefs. But *lis eft coram judice*, and as the appeal is

made

made to you, Sir, no doubt but the public will expect your determination.

" Cor. 2. Pag. 7. V, the velocity with which " the water flows through the hole, is as " $\sqrt{\dfrac{V^2 H}{V^2 - v^2}}$, or as $\sqrt{\dfrac{A^2 H}{A^2 - a^2}}$, or as $\sqrt{\dfrac{IG \times H}{IG - IH}}$, " or as $\sqrt{IG}$.". Now, Sir, in my judgment, and for the reasons given already, for the Doctor to prove all these expressions to be equal is absolutely impossible. But you see that he stands to this, that the velocity of the water at the point of efflux G, is not equal to the velocity which would be acquired by accelerating gravity in falling the whole perpendicular height of the water H G, but a velocity which would be generated by falling from a greater height I G.

" Cor. 3. If A be $= a$, V will be nothing. " For when a is equal to A, v will be equal " to V by this proposition ; but $a = A$, and " $v = V$, H will be nothing by Cor. 1. and " consequently V will be nothing by Cor. 2."

It is surprizing to see how dexterously the Doctor draws one absurdity and contradiction from another, and runs on in a long train of false consequence. Having created the imaginary quantity I H, he makes it serve all his purposes, and bring him off at every pinch. For all his false reasonings depend upon his supposing, the accelerating velocity to begin, or to be in its first nascent or last evanescent state, at some assignable point or other above the top

B 2   surface

furface of the water, which is plainly im-
poffible.

In the next Corollary 4. he proceeds upon
the fame falfe principle, and deduces it from
what has been confuted already, which there-
fore I need not take any farther notice of. But
his fifth and laft Corollary is more remarkable,
and indeed ftrange as from him, becaufe it
comes pretty near the truth. It runs thus, Cor. 5.
" If the hole be exceeding fmall in compari-
" fon of the upper orifice; the velocity with
" which the water flows through the hole,
" will, without any fenfible error, be equal to
" the velocity which a heavy body will acquire
" in falling in *vacuo*, through a fpace equal to
" the perpendicular height of the water in the
" veffel, *i. e.* V will, without any fenfible error,
" be equal to the velocity acquired in falling
" in *vacuo* through a fpace equal to H."

Now all this is very true, and needed no
proof from the Doctor's principles. It is the
law of accelerating gravity demonftrated by Sir
Ifaac Newton, and confirmed by ten thoufand
experiments. But that this is true only in this
particular cafe, and not in all others of the
fame kind where accelecrating gravity takes
place is falfe, and could only have been con-
cluded from falfe principles, grounded on falfe
facts, or very inaccurate experiments. Whe-
ther the cylinder, as above, fubfides or defcends
altogether, or whether a part of it, the column
fuperincumbent over the orifice, be fuppofed to
fall

fall by the accelerating force of gravity, the cafe is ftill the fame, and the velocity acquired at the point G will be the fame. For it is well known, that all bodies, whatever their quantities of matter or denfities are, will acquire equal velocities, and defcribe equal fpaces in equal times, while they are acted upon only by the power of gravity without Refiftance.

But after all, there feems to be fomething in this cafe that Dr *Robinfon*, as great a mafter as he is of Sir *Ifaac Newton*, has not fufficiently confidered. It is certain, that when water flows in canals or pipes, and runs out of a greater fection into a lefs, or *vice verfa*, the velocities muft be every where reciprocally as the fections, or equal quantities muft flow through every fection in the fame time: For otherwife the æquilibrium and communication of the fluid muft be neceffarily deftroyed. And from hence it is evident, that water thus communicating and flowing through pipes or canals, does not obferve the law of accelerating gravity, fince that has no regard at all to any fections or communication of fluids, but to an accelerating perpendicular defcent only.

That the motion of water thus communicating in pipes, is not at all governed and directed by the power of *gravity*, but by a conftant uniform *preffure*, is farther evident from hence, that this preffure from a given height, which is the fole caufe of the motion, acts *undiquaque*, or in all poffible directions, alike. Water
thus

thus preffed will afcend upwards againft the force of gravity, with the fame velocity and impetus that it will defcend or flow horizontally, where the perpendicular height or preffure is the fame.

The fame thing may be thus alfo demonftrated ; that the motion of water arifing from preffure is inftantaneous, and not acquired in time, as all accelerated motion muft. And this motion under the fame velocity, is as inftantaneoufly communicated to a greater quantity of the fluid as to a lefs, to the whole ocean as to a cubic inch. For whatever quantity of matter may be put into motion by fuch a Caufe, any force, weight, or refiftance, that is fufficient to fuftain a perpendicular cylindrical column of water of fuch a bafe and height, will inftantly deftroy or ftop the whole motion, and bring the fluid to perfect reft, let the quantity before in motion be more or lefs, and whether this force or refiftance be applied immediately to the orifice at the point of efflux, or at any given diftance, where the water has been conveyed in a train of communicating pipes for many miles.

It is well known, that water, how far foever it may have been conveyed horizontally in pipes, will rife perpendicularly to the fame height with that from which it is preffed. At this height the two oppofite columns fuftaining and counterballancing each other, the fluid will reft and ftand in æquilibrio. But if the water in the counter-

counterpoifing pipe be raifed a little higher or above the fountain head from which it is preffed, this additional quantity, though ever fo fmall, will turn back the whole body of the communicating water though ever fo great, and give it a contrary direction. This is very evident, and neceffarily follows from the firft and grand fundamental law of Hydroftatics and fluid preffure; which is, that any body or quantity of a communicating fluid, will always retain its æquilibrium, or equal preffure; and this inftantaneoufly, as foon as any part of the fluid fhall be preffed upon a given furface.

And from hence we may fee, the effential difference between the powers of *preffure* and *impulfe*, and of motion excited or communicated by one or the other. Motion communicated by impulfe, continues after the impulfe in the fame quantity, till it is deftroy'd by fome equal force in a contrary direction; but motion communicated by preffure immediately ceafes, as foon as that preffure is taken off, or otherwife fuftained. This is evidently fo in fluids, as we have feen already, and the cafe is the very fame in folids. Thus let any folid body lie at reft upon a plain table; if it be ftruck or impelled by an ictus or blow, it flies off in the direction of the impulfe, and retains its motion till it is ftopt by fome contrary force. But if without a blow, or impulfe, it is only pufhed or preffed with any given velocity, that motion is immediately fufpended, and the body remains at reft

upon

upon taking off the preſſure, or removing the hand or finger that forced it forward. But farther, in communicating motion by impulſe, the impelling body loſes as much motion itſelf as it communicates to the other; in conſequence of which, the quantity of motion is equal both before and after the impulſe, but the velocities will be reciprocally as the quantity of matter moved. But in motion communicated by preſſure, the caſe is perfectly different, for here the velocity ariſing from a given preſſure or perpendicular height of the fluid will be equal, whether the quantity of matter to which the motion is communicated be more or leſs. Theſe things are ſo very plain, that I ſhould have been almoſt aſhamed to have inſiſted on them to you, Sir, or to have troubled the public with them, had not Dr *Robin-ſon* made it neceſſary, by overlooking and ſetting aſide all the true laws of fluid motion, and introducing new and falſe ones of his own.

Now to apply this to the ſeveral caſes of the motion of water communicating in pipes, I ſhall ſuppoſe,

1. That a perpendicular deſcending pipe to be always kept full of water to a given height, ſhall communicate with another horizontal pipe of the ſame diameter or ſection. And in this caſe, ſince the velocity in the horizontal pipe is fixed and determined by the height of the perpendicular preſſure; and ſince the ſections in the perpendicular and horizontal pipes are equal, it is evident that the fluid both in its perpendicular
dicular

dicular defcent, and in its motion horizontally muft move equably and uniformly, or defcribe equal fpaces in equal times, for otherwife the æquilibrium and communication of the fluid could not poffibly be maintained, which would be contrary to the firft and fundamental law of fluidity.

But, 2. Let the perpendicular height, and the fection of the horizontal pipe ftand as before; and let the fection of the perpendicular pipe be increafed in any given proportion, or let its fection or furface be fuppofed infinite. And in this cafe, the velocity of the water thro' the horizontal pipe will be the fame as before, as being preffed from the fame perpendicular height, but the equable or uniform defcent of the water in the cylinder will be lefs in the reciprocal ratio of the fections, or as the fection of the horizontal pipe to the furface of the defcending water. And this is the ftanding unalterable law of water communicating in pipes, and flowing out of a larger fection into a lefs, or *vice verfa*. In which cafe the motion of the fluid at equal fections muft be always equal, and the law of accelerating gravity cannot poffibly take place.

3. The reft ftanding as before, let us fuppofe the horizontal pipe to communicate at the other end with another perpendicular pipe of equal diameter or fection with the horizontal one. It is evident in this cafe, that when the water comes to this pipe it muft afcend perpendicularly,

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pendicularly, and will afcend to the fame height or horizontal plain from which it was originally preffed. It is manifeft, that upon this fuppofition the velocity of the water through the whole communicating fyftem muft be equally retarded in proportion to the times, as a body falling downwards by the fole force of gravity would be accelerated in the fame proportion; and therefore the times of afcent and defcent in both cafes muft be equal. And this is the true analogy betwixt preffure and gravity, though they are two perfectly diftinct and different power or laws of nature. But if the fection of the perpendicular afcending pipe be greater or lefs than that of the horizontal one, the velocity of the afcending water will be accordingly greater or lefs than the horizontal velocity, and this reciprocally as the fections. But every one knows that where the fpaces defcribed are the fame, the times will be reciprocally as the velocities; that is in this cafe directly as the fections. And therefore, the time in which water will afcend to the fame height from which it is preffed, may be greater or lefs than the time which a heavy body would take in falling through the fame fpace, and this in any given proportion: Namely, in the proportion of the fection of the afcending to that of the horizontal pipe.

4. Let us fuppofe any horizontal pipe as before, to communicate with any one or more pipes lying in the fame plain. The original pipe which brings the water from the fountain I fhall
call

call the primary pipe, and the other communicating pipes the secondary ones. This being supposed, the velocity of the fluid in the secondary pipes will be to its velocity in the primary pipe, or at the point of efflux, as the section of the primary pipe to the sum of the sections of the secondary pipes. For if it was otherwise, the æquilibrium and communication of the fluid could not possibly be maintained, by the primary governing law of fluidity; but if the sections of the secondary pipes are not all equal, the velocity of the fluid through any one of those pipes, will be to the common velocity through the whole system, as the sum of the sections of all the other communicating pipes, is to the section of this particular pipe, by the same general law.

5. If the secondary pipes are not situated in the same horizontal plain, but are inclined in any angle above or below the horizon, this will alter the quantity of the perpendicular pressure, which will be more if the inclination of the pipe be below, and less if above the horizontal plain. And here this increase or diminution of the pressure or perpendicular height must be always as the sign of the angle of inclination. Let the length of the pipe be put equal to radius, and then the sine of the angle of inclination will be the perpendicular distance above or below the horizontal plain, and the co-sine of that angle will be the horizontal distance from the point of insertion. Suppose then any such pipe to be in-

C 2   clined

clined to the horizon either above or below it by an angle of 30 gr. and then will the length of the pipe, the horizontal diftance from the point of infertion, and the perpendicular increafe or diminution of the fluid, be refpectively as the numbers 10000000, 8660254, and 5000000. Now let the perpendicular height of the fluid above the point of efflux be called p. the elevation or depreffion above or below the horizon by fuch an inclination s, and then the increafed

or diminifhed velocity will be as $\frac{}{p+s}^{\frac{1}{2}}$; and

here it is manifeft, that this increafed or diminifhed preffure muft affect the velocity through the whole communicating fyftem, and at every fection or fum of fections proportionally, or otherwife the æquilibrium and communication of the fluid muft be deftroyed and broken off.

Thefe things are evident, and follow demonftrably *à priori*, from the fundamental governing law of fluidity, *viz*. the æquilibrium and communication of the fluid through all its fections ; which Dr *Robinfon* had not the leaft regard to, while he was fetting afide by experiments all the laws of fluid motion. And now after what has been faid, I may proceed to the folution of the following Problem.

PROBLEM.

PROBLEM.

*The perpendicular height of a fluid, and its section
at the point of efflux, being given, to determine
the velocity and impetus of the fluid at any
other given section.*

It is here evident at first sight, that a column
whose base is the orifice, and its height the per-
pendicular height of the fluid above the point of
efflux, is the whole quantity of pressure which
the whole communicating fluid at every section
sustains. For take away this, or let it be other-
wise ballanced or supported, and the whole mo-
tion immediately ceases, whether the quantity of
matter in motion was more or less. Let the per-
pendicular height be called p, the effluent velo-
city v, and the section or area of the orifice a;
and then $p\,a$ will be the whole pressure of the
fluid, or moving force which generates and con-
tinues the motion. Since while this remains
the motion is constantly and uniformly kept up,
but being removed or counterballanced the mo-
tion instantly ceases, and the fluid is at perfect
rest ; but the velocity is always in the subdupli-
cate ratio of the perpendicular height, and con-
sequently the height as the square of the velocity.
And therefore $a\,p$ is as $a\,v^2$, that is, the force
or impetus of the effluent fluid is as the square
of the effluent velocity multiplied by the ori-
fice ; or the same thing may be thus otherwise
demon-

demonstrated : The force or impetus of a fluid is as the quantity discharged, through a given section in a given time, multiplied into the velocity ; but the quantity discharged in a time given, is as the velocity into the section, or as $a v = q$, the quantity of matter therefore being as $q v$, the quantity into the velocity will be equal to $a v^2$, as before. And that the section being given, the impetus will be as the square of the velocity, might be thus proved, under a double velocity, a double quantity of the fluid must pass through the same section in the same time, and therefore the force or impetus must be quadruple, or as the square of the velocity, at equal sections, and universally in a ratio compounded of the section and square of the velocity. *q. e. i.*

COROLLARY.

From hence it follows, that when a fluid after having passed the original section at the point of efflux, runs into a pipe or pipes of unequal sections; the force or impetus at any such section, or sum of sections, will be as the velocity directly. For since in this case, the velocity at the several sections, or sum of sections, must be inversly as those sections, the rectangle of both expressing the quantity of matter will be always a ratio of equality, and may be taken as unity, which will reduce the ratio $a v^2$, or $q v$ to v, the velocity directly, *q. e. d.*

These

Thefe things, Sir, appear evident to me, and the neceffary confequences of the law of preffure and communication of fluids. I had infifted on them more largely in my *Mechanical Practice* of Phyfic, about four years ago, which Dr *Robinfon* did not think fit to take any notice of. And, indeed, it was then too late to mend or alter any thing without throwing it all off, and giving up the whole fcheme; the contrary to which he has chofen, and appealed to you.

But as the Doctor goes on ftill upon the fame falfe fuppofitions, and to draw other confequences and deductions from them, I fhall purfue him no farther in his peculiar theoretic way; but proceed to confider the anfwers which he has offered in this letter to you, to the feveral difficulties and objections that I had urged againft him in my *Mechanical Practice of Phyfic.*

Lett. pag. 31. " This may fuffice concern-
" ing the motions of water through orifices and
" pipes. I fhall now proceed to Dr *Morgan's*
" *Remarks*, and to fhew that they have all been
" occafioned by his not having duly attended
" to what Sir *Ifaac* and I delivered concerning
" thefe motions." The Doctor is doubtlefs at liberty to place himfelf on a level with Sir *Ifaac*, and me as much below himfelf as he thinks fit.

" *In pag.* 68. *l.* 4, 5. The Doctor fays that
" F will ever be as $D^2 H$; whereas, had he at-
" tended to Prop. 36. lib. 2. *Newton.* He
" would have feen that F will never be as
" $D^2 H$, but when the area of the hole is infi-

I   nitely

" nitely little in comparison of the area of the
" surface of the water in the veſſel, and the
" pipe lies parallel to the horizon. For I have
" ſhewn from that propoſition, that the force
" which can generate the motion of water flow-
" ing through a hole, is equal to the weight of
" a cylinder of water whoſe magnitude is a
$\times$ IG, or $a \times \dfrac{A^2 H}{A^2 - a^2}$." But I think I have

proved this to be falſe, and therefore I hope
the Doctor will not father the abſurdity upon
Sir *Iſaac*, or pretend to deduce it from the pro-
poſition he refers to, though he ſhould be forced
to come down a ſtep or two below that great
maſter of ſcience. The Doctor goes on, " 1.
" The Doctor ſuppoſes D and H to be given,
" and conſequently the moving force which is
" as D² H to be given ; in which caſe V will
" be as $\sqrt{\dfrac{1}{L}}$. And then affirms, that if L be
" infinitely ſmall, V muſt be infinitely great,
" and if L be infinitely great, V muſt be infi-
" nitely ſmall."

This is no conſequence which I have forced
upon the Doctor ; it is his own deduction,
Vol. 1. Prop. 1. Cor. 1. That every thing elſe
being equal, V will be as $\sqrt{\dfrac{1}{L}}$. And this being
ſuppoſed, it is evident to every one at firſt
ſight, that V, or the effluent velocity under a
given preſſure, may be increaſed or diminiſhed

in

in any given ratio, or *in infinitum.* To evade
this, Dr *Robinson* runs into a long impertinent
harangue about the infinite divisibility of matter,
and the relative nature and different orders of
infinites; all which is no more to the purpose,
than if he had gone about to prove, that his
proposition muſt be true, and my objection in-
valid and of no force, becauſe the earth moves
about its axis, or becauſe grafs grows on the
tops of houſes. If V be as $\sqrt{\dfrac{1}{L}}$, it is very

plain, that where L is infinitely great, V muſt
be infinitely ſmall; and where L is infinitely
ſmall, V muſt be infinitely great; and that be-
tween theſe, the velocity V may be increaſed
or diminiſhed in any finite aſſignable ratio; and
this, without entering into the ſpeculation about
the different ranks and orders of infinites. And
from hence it muſt neceſſarily follow, as I had
urged upon him, that the effluent velocity of a
fluid is not that which would be acquired in
falling through the whole perpendicular height
of the fluid above the point of efflux, but may
be any other velocity more or leſs, finite or in-
finite, aſſignable or not aſſignable. If Dr *Ro-
binſon* cannot ſee this I ſhould pity him, and
think him uncapable of conviction.

But Dr *Robinſon* goes on; " 2. The Doctor
" pag. 69. ſuppoſes D to be given, and H to
" be proportional to L; in which caſe the velocity
" through the pipe will be given; but however
" ſtrange this may appear to this Gentleman,

D   I can

" I can affure him, that I have found it true by
" experiments." But I hope the Doctor will
no longer urge his own experiments againſt all
nature, fact, and experience. For if this prin-
ciple and law of motion will hold good in
Ireland, and under his management, I dare ven-
ture my life on it, that it will hold good no
where elſe, or under any other direction. You
ſee, Sir, he here aſſures us, that the effluent
velocity will be the ſame every where, provided
that the length of the horizontal conveying pipe
be but proportional to the increaſed or dimi-
niſhed height of the fluid. I ſhall make no
farther remarks upon ſuch an abſurdity, and a
thing ſo contrary to all fact and experience, as
known to every mechanic, becauſe I would not
preſs the Doctor more than the argument itſelf
forces me to. But the caſe lies before you, Sir,
and no doubt but the public will wait for your
deciſion. Farther, the Doctor ſays, pag. 35.
" In his third conſequence, pag. 70. drawn
" from V being as $\sqrt{\dfrac{DH}{L}}$. This author ſup-

" poſes L to be given, and D to be as $\dfrac{I}{H}$; in

" which caſe the velocity will be given. He
" thinks this to be very abſurd, as I gather
" from his fifth conſequence, in which he ſays
" expreſsly, that D and L have nothing to do
" in the matter, and cannot alter the velocities
" at all. But in this he is greatly miſtaken,
" for both the diameter and length of a pipe
" affect

" 5. In his fifth and laft Confequence, *pag.*70.
" he fuppofes D and L to be given ; in which
" cafe V will be as √ H.   This he allows to be
" true, and fays it is the true law of accelerating
" gravity and preffure, as determined by *Newton.*
" But in this he is miftaken, for V is not as

" √ H, according to *Newton*, but as $\sqrt{\dfrac{A^2 H}{A^2 - a^2}}$,

" by *Cor.* 2. of the foregoing propofition." But
I muft tell the Doctor too in my turn, that he
is greatly miftaken, and that *Newton* has no
fuch principle or law of velocity, as he pretends
to deduce from him, nor any thing like it.   I
have proved already, and fhall farther prove,
that fuch a fuppofition muft be attended with
infinite abfurdities and falfe confequences, which
I am fure Sir *Ifaac Newton* could never be guilty
of, nor have in his thoughts.   The Doctor's
great law and rule of velocity is this, that V, the

effluent velocity of the fluid, is as $\dfrac{A^2 H}{A^2 - a}$. This,

as he affures us, is a plain and obvious confe-
quence of *Prop.* 36. *lib.* 2. *Newton.*   I muft
confefs, that I could never fee this as a plain and
obvious confequence, or any confequence from
that propofition in *Newton* ; nor did I ever meet
with a man before who could fee it, or pre-
tended to fee it.   The Doctor, I hope, will ex-
cufe my dulnefs, efpecially while I am in fo
much good company.   But I muft debate this
matter a little farther with him, whofe autho-
rity

rity from Sir *Isaac*, I cannot much depend on.

It is plain then, I think, that water continually flowing through a perpendicular pipe, may communicate with a horizontal pipe or system of pipes, whose section, or the sum of their sections, may be equal to, or greater or less than, the section of the perpendicular vessel, or pipe, through which the water descends to supply the rest. To deny this would be to deny all nature and experience, and to be contradicted by every mechanic and every man. Now this being supposed, it is very plain, that *a* and A, in Dr *Robinson*'s notation, may bear any proportion one to another, and *a* may be equal to, or greater or less than A. For it is not the capital letter that makes A always greater than *a*, as these symbols have been introduced only to denote the perpendicular from the horizontal direction; but it is impossible to make any limitation here upon any principles of nature and fact, whether the perpendicular or horizontal communicating pipes shall be largest. If then A and *a* may bear any proportion of equality, or greater or less than one another, and this mutually and reciprocally, while the perpendicular height or pressure remains the same; all the absurd consequences which I have charged upon the Doctor must hold good. But the Doctor denies that this can ever be so, and I must now consider the reasons he has given for it.

In

[30]

In the propofition with which this argument is introduced at the beginning of the Letter, the refult of the Doctor's reafoning, keeping to the fame notation is this, that $V = \dfrac{V^2 H}{V^2 - v^2}$, or $\dfrac{IG \times H}{IG - IH}$, or I G, *fays the Doctor*. But becaufe he has not proved this, I fhall endeavour to prove it for him.

I had fhewn before that I H is a quantity merely imaginary, and that to fuppofe any accelerating velocity to begin above the top furface of the water is abfurd and impoffible in fact. Setting afide therefore his imaginary I H, and v, which in this cafe can never be any thing at all; it is plain $\dfrac{IG \times H}{IG - IH} =$ H, and IG $=$ HG $=$ H. And if the Doctor has reafoned right $\dfrac{A^2 H}{A^2 - a^2}$, muft be always $=$ H. And yet he owns that *this can never happen but when* a *is infinitely fmall*, *i. e.* it can never happen at all. How the Doctor fhould come by any fuch expreffion of velocity from Sir *Ifaac Newton*, I can not imagine; but if he came by it honeftly, I doubt it was not wifely. From what has been faid, Sir, I think it will follow, that all the abfurd and contradictory confequences which I had charged upon Dr *Robinfon* muft hold good, and that he built a whole theory of the *Animal Oeconomy*, and the motion of fluids upon mere imagination

imagination and fiction. It is well known to every mechanic, and almoſt to every man, that water will any where riſe perpendicularly to the ſame height from whence it is preſſed, or to a level with the ſurface of the baſon, reſervoir, or fountain, from which it is fed. And whether we ſuppoſe the length and diameters of the horizontal pipe, or communicating pipes, to be more or leſs, the caſe is always found in fact to be the very ſame, and the water will riſe to the ſame level or perpendicular height, after it has been conveyed in communicating pipes three or four miles as it would juſt at hand. And this ſhews that the impetus of the fluid, with reſpect to its effluent velocity, is not increaſed or dimi-niſhed by the length or diameter of the horizon-tal pipe, or communicating train of pipes; for were it ſo, the water would aſcend perpendi-cularly either above the ſurface from whence it is preſſed, or reſt below it; both which are impoſſible, and contrary to fact. Theſe are ex-periments which we ſee made every day in all conveyances of water, and do not in the leaſt depend on Dr *Robinſon*'s inventions.

But to give the Doctor an opportunity to exa-mine farther the truth and uſefulneſs of his theory, I ſhall here put a caſe and a problem upon it, to be ſolved at his leiſure, or when he writes next. Suppoſe then the baſon or reſervoir, which is always to be kept full of water to a cer-tain height, not to be a cylinder, but a truncated cone, of which the baſe or lower ſurface of the

water

water shall be taken at pleasure, but the top or
upper surface shall be exactly equal to the area
of the orifice or section through which the water
is to flow. This may as well be supposed as a
reservoir of any other figure or form, and would
answer all the same purposes in the pressure and
communication of fluids. In this case the
Doctor's A, would be exactly equal to *a*, or the
upper surface of the water equal to the orifice be-
low. Now this being supposed, I would willingly
know upon the Doctor's principles, what the
effluent velocity of the water must be, whether
finite or infinite; if infinite, of what different
rank or order of infinites ; or if finite, what
its assignable quantity must be ?   I hope the
Doctor will not say, that in this case, *there could*
be no motion at all, and that both the perpendi-
cular height and effluent velocity would be destroyed.

The Doctor had observed from Sir *Isaac New-*
ton, that when a stream of water flows out of
an orifice or pipe horizontally from a given per-
pendicular pressure, that the stream from the
point of efflux contracts itself, and is forced into
a conclal form, for a small distance, as about
the diameter of the pipe or orifice. And from
hence he imagines, that the cataract, or falling
column, contracts itself in falling, and does not
fill the orifice or pipe at the point of efflux ;
but leaves a cavity, or void space, between the
effluent stream, and the inward concave surface of
the pipe or orifice.   I can understand him in no
other sense, but least I should be thought to mif-
<div align="right">represent</div>

reprefent him, I fhall here quote him in his
own words.

" In obferving the motion of water flow-
" ing through a hole made in the fide of a
" veffel, we may perceive the vein not to
" fill the hole. Sir *Ifaac Newton*, in deter-
" mining this motion from experiments,
" found the vein, after it had paffed out of
" the hole, to grow fmaller and fmaller, till
" it came to a diftance very nearly equal to
" the diameter of the hole ; at which place
" he meafured the diameter of the vein, and
" found it to be to the diameter of the hole
" as 21 to 25. The area of a tranfverfe
" fection of the vein, at that diftance from
" the hole, is to the area of the hole ; as
" the fquare of the diameter of the vein, to
" the fquare of the diameter of the hole :
" that is, as 12 to 17 nearly. This con-
" traction of the vein, arifes from the na-
" ture of the motion of the water down the
" veffel ; for the water falls down from the
" top of the veffel through the hole, not
" perpendicularly but obliquely, its parts
" moving laterally as well as downwards
" from the obliquity of this motion ; it is,
" that the column of the defcending water
" grows narrower perpetually from the top
" of the water to the hole, and to a fmall
" diftance beyond it, leaving a little empty

E   " fpace

" space all round. On account of this
" contraction of the vein, lefs water flows
" out, and by confequence lefs motion is
" generated in a given time, than would be
" produced if the diameter of the water at
" the hole was exactly equal to the dimenfion
" of the hole, and as lefs motion is gene-
" rated, fo the moving force is likewife lefs ;
" being only equal to the weight of a
" cylinder of water, whofe magnitude is
" $\frac{12}{17}$ a H, when the hole is extremely fmall
" in comparifon to the upper furface of the
" water; whereas it would be equal to the
" weight of a cylinder of water whofe
" magnitude is 2 a H, if the vein filled the
" hole, and had no contraction beyond it ;
" and therefore the moving force is lefs than
" it would be if the vein filled the hole, and
" had no contraction beyond it, in the pro-
" portion of 12 to 17. But $\frac{12}{17}$ a H, 2 a H,
" is not as 12 to 17, but as 12 to 34, which
" the Doctor it feems had forgot. *Anim.*
" *Oecon. pag.* 14—16."

I have quoted the Doctor out in this rab-
ble, becaufe there are feveral uncommon,
and very extraordinary things to be obferved
upon it. Firft, he fuppofes that a cylin-
drical

drical column of water falling freely and without resiſtance, muſt contract itſelf in the fall, and be thrown into a conical, or rather hyperbolical form; but this is plainly impoſſible, and contrary to the nature of accelerating gravity. For let us ſuppoſe any given cylindrical column of water to deſcend freely, and without reſiſtance by its own gravity; and in this caſe it is evident, that as all the parts of the water, or every parallel ſurface from top to bottom, muſt begin to fall at the ſame time, every part of the water muſt deſcribe equal ſpaces, and acquire equal velocities in equal times; and therefore the water muſt ſtill retain its cylindrical figure, and cannot contract itſelf into leſs ſections by any power or action of gravity upon it in falling. For ſuppoſing this, the velocity of the water when it begins to contract itſelf, muſt be greater, and the ſpaces deſcribed greater than what could have been acquired by accelerating gravity in the ſame time, and this in the reciprocal ratio of the ſections. For every one muſt ſee, that while the communication of the fluid is continued, equal quantities muſt paſs through every ſection in the ſame time, and therefore the velocities muſt every where be reciprocally as the ſections; but this never happens but when the velocity is uniform and equable at equal ſections,

E 2   which

which is quite contrary to the law of accelerating gravity.

Alike abfurd in itfelf, and contrary to fact, is the Doctor's fuppofing, that the water does not fill the orifice or pipe, but that it contracts itfelf all through, and leaves an empty fpace or *vacuum* about the internal concave furface. But whoever will obferve the Phænomenon as above, will find, that the water exactly fills the hole or pipe within, and that it does not begin to contract itfelf but juft at the point of efflux, where it quits the pipe and begins to flow in open air. And from the point of efflux the external ftream continues to contract itfelf to the diftance of about a diameter of the pipe or orifice, as has been found by obfervation. The reafon of this contraction, the quantity of the perturbating force occafioning it, and the diminifhed momentum of the effluent fluid from thence arifing, I fhall now fhew.

It is commonly known, that when fmall open pipes are perpendicularly immerfed in water, the water within the pipe will afcend to a certain height above the external furface of the water ; and this height has been found by experiments to be always reciprocally as the diameter of the pipe. From hence it follows, that the caufe of this appearance,

and

and such suspension of the fluid, must be
always as the diameter of the pipe directly.
Though this is obvious enough, yet I think
Dr *Jurin* was the first that observed and
demonstrated it. And thereby evinced the
true cause of raising and sustaining such a
quantity of the fluid; but when water flows
through a horizontal pipe into the air, the pipe
is exactly in the same circumstance as be-
fore, for its end is immersed in water re-
maining in the open air, and the axis of the
pipe is perpendicular to the section of the
water, or stream flowing in air. And con-
sequently the attraction of the extream peri-
phery of the pipe at the point of efflux, must
have the same effect, and retain an equal
quantity of water, or lessen its progressive
motion in the same proportion as in the
former case. Now it is plain in fact, that
the perturbating cause which forces the effluent
stream to contract itself, acts as the diameter,
because this is always the distance to which
its action is continued; but the resistance to
the motion of the water at the efflux, must
be greater at the surface of the stream which
is contiguous to the attracting periphery of
the pipes, than at its axis where the attraction
is less; and this must necessarily force the
stream at its parting with the pipe into a
conical form. And in this case, such a
frustum of a cone will be discharged in the
same

fame time, that a cylinder of equal bafe and length would have been difcharged without any refiftance or perturbation of motion. Now the content of fuch a fruftum, will be to the content of a cylinder of the fame bafe and length, as the leffer diameter of the fruftum is to the diameter of the cylinder, as any one will eafily find by computation; which method of computation being very well known, I fhall not farther infift on it here. But the leffer or outer diameter of the fruftum is to the diameter of the cylinder the diftance of a diameter of the pipe, as 21 to 25, or as one to the biquadratic root of 2, as Sir *Ifaac Newton* has found by experiments. And therefore this muft be the proportion in which the momentum and quantity of the effluent fluid is leffened from fuch a perturbating caufe; the proportion of 1 to the biquadratic root of 2, is as 10000 to 11892, or nearly, and without any confiderable error, as 5 to 6, or, as Sir *Ifaac* has put it, as 21 to 25. And from hence it muft follow, that the velocity and quantity difcharged under fuch a refiftance, is the fame that would be difcharged without any refiftance, by a velocity acquired in falling through a fpace that is to the whole perpendicular height of the fluid, as 1 to √ 2. For the velocity and momentum, by the refiftance at the orifice, being
diminifhed

diminished as 1 to 2¼ and the velocity ac-
quired by accelerating gravity being always as
the square of the space described the square
of 2¼ will be 2½, or √ 2 ; and therefore the
space described by accelerating gravity under
such resistance, is to the space which would
have been described without resistance as
1 to 2 ½.

From hence says the Doctor, *i. e.* from
*the water not filling the pipe through which
it descends, as less motion is generated so the
moving force is likewise less ; being only equal
to the weight of a cylinder of water whose mag-*

nitude is $\frac{12}{17}$ H. *Whereas it would have been*

*equal to the magnitude of a cylinder of water
whose magnitude is 2 a* H.

He here supposes, that by the resistance
the fluid meets with at the periphery of
efflux, its force or impetus is lessened in the

proportion of $\frac{12}{17}$ to 2, or as 6 to 17, which

is almost as 3 to 1.   But from whence he
should draw any such consequence I cannot
conceive. I think I have proved, that the
diminution of the force or impetus from the
cause above mentioned, is only as 1 to the
biquatratic root of 2. *i. e.* as 1000 to 1189,
or, as near as can be expressed in such small
numbers,

numbers, as 21 to 25. and therefore not as 12 to 34, or 6 to 17. But the Doctor's demonftrations are all new and extraordinary, and I believe there were never afore fuch yokes of *Irifh* bulls feen in *England*.

To all the abfurd and contradictory confequence, which I had charged on Dr *Robinfon*'s hypothefis of fluidity, he contents himfelf with this general anfwer, *That however abfurd and contradictory thefe things may appear to me, he has proved them true by experiments.* But the Mathematicians, I believe, will not put the credit of his experiments againft demonftration, and the moft common notorious and convincible facts to the contrary : Facts known to every Mechanick and Engineer, and to every man in the leaft converfant with the conveyance and communication of water in pipes. Indeed the Doctor's errors in thefe matters are fo very grofs and palpable, that I was almoft afhamed to write any farther againft him, and fhould not have done it, Sir, if he had not publifhed his books in *London*, and in fuch a manner appealed to you.

This may fuffice for the Doctor's *Mathematical*, I fhall now proceed to make fome obfervations on his *Mechanical* reafonings, and fhew, that he has been equally unhappy in this.

this. He had undertaken to prove, that the life of animals, the heat and motion of the blood, and *the action of fire, are preserved and kept up by the means of acid particles in the air, which acid is mixed with the blood in the lungs.* To prove this, the Doctor observed, that air, and a continual supply of fresh air, is neceffary to preserve the life of animals, and maintain and keep up the action of fire. He likewise obferved, that a mixture of acid and alkalious liquors will ferment together, fo as to acquire a confiderable heat, and fometimes even actually take fire. And from thefe facts he concludes, that the heat and motion of the blood, and the life of animals muft proceed from the fame caufe, and be owing to the mixture of an acid with the blood in the lungs. Now I denied this confequence, becaufe I could not fee it, and I gave feveral reafons to fhew, that the thing here concluded cannot be true in fact; and I cannot perceive, Sir, that the Doctor has removed any one of thefe objections, or faid any thing to the purpofe in this his Letter to you. Could he have proved either that there are acids and alkali's mixed with the blood, or that heat and motion can arife from no other caufe, he might have had fome fhow for his confequence; but for want of this he has left it without any

F   proof

proof or foundation at all, either in reafon or fact.

The learned, and juftly celebrated *Boerhaave* in all his chemical procefles, and by the moft numerous experiments, has proved, that there is nothing of an acid or alkali contained in the blood, or any animal fubftance in their natural ftate, but that they are all of a neutral kind, neither acid nor alkaline, and will not ferment or acquire any heat with one or the other. And from hence it is plain, that though we may take in great quantities of both acids, and alkali's into the ftomach, yet they are all foapified, or changed into a *tertium quid* by the action of the ftomach, and concoctive power of the other glands, before they can be brought to the blood, or be made fit for the purpofes of animal life and motion there. But the Doctor concludes, that becaufe acids and alkali's when mixed out of the body will produce a confiderable heat and motion; that therefore the cafe muft be the fame in the body where no acid or alkali could ever be found; as if there could be no other caufe of heat and motion but fuch a chemical fermentation. It is a plain and well known fact, that any elaftic combuftible matter by violent motion, or ftrong friction, will acquire heat, and even take fire. When therefore two
<div align="right">fticks</div>

sticks are rubbed together till they catch
fire, or when the axle or wheel of a coach,
or waggon, by violent motion and friction
take fire, is this ignition owing not to the
motion, or elaftic friction, but to certain acid
particles in the air ? Can any thing be more
inconfequent, when an acid would imme-
diately deftroy the action and extinguish the
fire : But the Doctor having got one caufe
of heat and motion, prefumed it to be the only
one, and that the fame thing muft alway
happen where the fame caufe cannot poffibly
act.

That the air is impregnated with acid,
alkalious, and faline particles, cannot well be
doubted, becaufe the atmofphere is con-
tinually drawing up and receiving all forts of
effluvia from the earth and fea ; and the
Doctor might have made any one of thefe the
caufe of the heat and motion of the blood as
well as the other ; but acids of all the reft have
the leaft claim to it, becaufe they fooneft
extinguish fire and deftroy its action. And
it is well known, that acids or acidulated
liquors, are always given inwardly to cool
the blood and abate its impetus, and never
to increafe its heat and motion ; and though
the air muft be always impregnated, more or
lefs, with effluvia of foreign extraneous matter
of all forts, yet the lefs it is thus faturated,

or

or the finer and purer it is, the better it certainly is, the fitter for breathing, and all the purposes of animal life, as well as to preserve and keep up the action of fire. This is what all experience testifies, and no new hypothesis can ever set it aside.

To my saying, that all fluids, acids as well as others, except oil, will extinguish fire; and that acids will check and extinguish fire sooner than common water. The Doctor replies, " And what then ? Will it from " thence follow, that a nitrous acid cannot, " when mixed with some oils in a certain " proportion, produce fire and flame ? By " no means". No, surely *by no means*, nor did ever I say or imagine any such thing. But I think I have proved, that the cause which the Doctor assigns for the heat and motion of the blood cannot be the true cause. But to close up the whole argument, the Doctor adds, pag. 54 and 55. " And " I am still farther convinced of the necessity " of an acid in the air to preserve the life of " animals, when I consider the insufficiency " of all other accounts of respiration. As " to the use assigned to the air by this Gen- " tleman; namely, *That it serves as a proper* " *exhaling medium, to receive and carry off* " *these copious discharges of humid effluvia, or* " *moist vapours, which all living creatures,*
" *and*

" *and all combuftible matter under the action*
" *of fire, are inceffantly emitting and throwing*
" *out* ; I muft beg leave to tell him, that
" this is an old trite hypothefis, without any
" the leaft foundation from reafon and ex-
" periments. For vapours and exhalations
" are not thrown off from humid bodies,
" by any virtue in the air, but by the re-
" pulfive powers of their particles, when by
" the action of heat, they are once fepa-
" rated from the bodies, and are got beyond
" the fpheres of their attractions, and of
" the attractions of one another. For this
" repulfive power will carry off the fepa-
" rated particles, as well *in vacuo* as in the
" open air".

I muft own that this account of the afcent
of vapours in air, which would not afcend *in
vacuo*, or in an air very much rarified, is
altogether new, and what, I fuppofe, the
world muft be obliged to Dr *Robinfon* for.
But that vapours will not afcend *in vacuo*, or
in an air very much rarified, and that in this
cafe the fire can have no action at all, is a
matter of fact and common experience, and
does not depend on any hypothefis old or
new. The Doctor owns that air is neceffary
to the life of animals, and to the action of
fire ; but he thinks that this neceffity does
not arife from the air itfelf, either on the
account

account of its weight or elasticity, but is owing to a certain acid contained in it. What reason he has for this has been considered already ; but he now gives it as his opinion farther, that the humid suffocating vapour is not raised from fire, or the body of animals by any virtue in the air, but by the repulsive powers of the particles, which perhaps may appear as extraordinary as the other. It is evident in fact, that whatever keeps the fire clear and free from any humid vapour, or suffocating damp, maintains its action whether it be the air itself or any other cause ; but if the fire was kept clear, and its vapour thrown off by the repercussive power of the particles this must be better done, and the vapour must be thrown off quicker and more copiously *in vacuo*, or in a very thin and rarified air, than in the common heavier and denser air ; because the particles of the vapour in this case, would meet with less resistance after the percussion given ; and on this hypothesis the fire would burn stronger *in vacuo* than in open air. But every body knows this to be false in fact, and therefore the hypothesis on which it is grounded must be false. What is it that raises and suspends any vapour in air, but its tenuity, as being specifically lighter than the air at the earth's surface ? And therefore any such vapour or effluvium must ascend in air,

and

and will be there fufpended and fuftained, where the air is of the fame denfity or fpecific gravity with itfelf. But when any fuch vapour comes to be condenfed by many of its particles uniting in one, fo as to become fpecifically heavier than air, they muft neceffarily fall down again by their own weight to the earth's furface. But this perhaps the Doctor will call an *old trite hypothefis*, and may not like it becaufe there is no difcovery in it. It is plain, I think, in fact, and to all experience, that when fire goes out, it is either for want of fuel, or any farther matter to work upon, or becaufe the air about it is too much heated and rarified to carry off the fmoak, or too much faturated with fome humid fuffocating vapour before; but whether this reafoning will hold good or no, or how far Dr *Robinfon's* new hypothefis may be preferable to fuch plain facts, muft be left to farther enquiry, and to your arbitration, Sir.

This is what I thought fit to reply to the Doctor's defence againft my former objections. But I fhall now proceed to make fome farther obfervations on other parts of the work which I then took no notice of. And here I think, in general, that this learned Gentleman, under a fhow of the utmoft accuracy, has run into the greateft darknefs and confufion;

fufion; of which I shall give several inftances.

Dr *Robinson* in his IVth section, which takes up the six last propofitions of the first volume, has made a great number of the most curious and accurate experiments, about the ftrength of hairs of different forts and fizes, taken from the heads of old women and young; and this, by meafuring, ftretching, and contracting, wetting and drying them, &c. but in the application of thefe experiments and obfervations to the *Animal Oeconomy*, the Doctor, as ufual, has been very unhappy.

By the ftrength of an animal fibre, it is plain, that he means the weight it can bear without breaking, or being fo far over ftretched as to lofe its elafticity or reftitutive force. And to determine this, he has fcarce omitted any experiment that could be made upon hairs, except fplitting of them, which to have done exactly through the axis might perhaps have been difficult even to him, for whom nothing feems too hard. He has tried experiments upon the different ftrength and diftractility of hairs, which he adjufts to the 100th, or 1000th part of an hair's breadth in the decimal or algorithmick way. But, *cui bono*, to what
purpofe

purpofe is all this? For it is evident that none of his experiments can be tried up-on any of the animal fibres within the bo-dy, which are to act any part for mufcular motion; will he wet or lubricate thefe with inflammatory fpirits, weak or ftrong acids, chymical oils, folutions of falts, and above fixty things which he has named, to which there is nothing analogous in an animal body? Befides, it is well known, that the hair is an excrefcence, as much as the nails, and has no more to do with animal motion, either in its natural ftate, or any artificial ftate that it can be put into; efpecially by the application of fuch fubftances, as cannot confift with ani-mal life or motion. But I fhall here fet down the Doctor's 35th propofition, to fave *himfelf and me fome trouble*, becaufe it may ferve as a fpecimen for all the reft. *Prop.* 35. *p.* 287. "If an animal fibre be extended "by a force acting uniformly upon it, "its ftrength will be directly as that force, "and inverfely, as the extenfion caufed by "it in a given time: and if the fibre be "of a given fort of matter, and its extenfion "caufed by the force in a given time, be "fmall; its ftrength then will be very near-"ly, or the fquare of its diameter directly, "and as its length inverfely. If S denotes "the ftrength of a fibre, F the extending "force, E its extenfion, caufed by the uni-

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" form action of that force in a given time,
" D its diameter, and L its length; then S

" will be as $\dfrac{F}{E}$; and if the fibre be of a

" given fort of matter, and its extenfion be

" fmall, then S will be very nearly as $\dfrac{D^2}{L}$."

Now here every one muſt fee at firſt glimpfe, that when all thefe if's and fuppofitions come to be applied to any particular cafe in the *Animal Oeconomy*, nothing at all will be given, and confequently, nothing can be found. Let the Doctor take any mufcle, or mufcular coat in an animal body, and let him tell me the length and fection of its fibres, what force is neceffary to give it a certain diftenfion in a given time, and to what degree an animal fibre in a living body, may be capable of reſtoring itfelf in a given time, after it has been ſtretched to a given quantity, by a given force, and in a given time. And when he has done all this, which is impoffible to be done, fomething or other may be determined, which for want of data, cannot now be found. And this is conſtantly the gentleman's way, firſt to fuppofe things given, that are not, and cannot be given, and then fhew how to find out what we cannot now difcover, for want of the neceffary data.

But

But in *Prop.* 41. which makes the firſt of the ſecond volume, the Doctor comes to apply this perfectly new hair-theory, to the different degrees of the tenacity and tenuity of the blood. *The tenacity of the blood, he ſays, is increaſed by things which ſtrengthen the fibres, and leſſened by things which weaken them.*

1. *Cold ſtrengthens, and heat weakens animal fibres, and cold increaſes, and heat leſſens the tenacity of the blood.* But it is ſtrange that the Doctor here, ſpeaking of the blood in general, ſhould not diſtinguiſh between the two different parts of it, the *ſerum* and the *craſſament,* or between the water, and the red concreted and denſer globules. Since it is plain, in fact, with regard to heat and cold, that what attenuates and fluxilizes the one, will render the other more viſcid and tenacious. In very cold languid conſtitutions, we find the blood runs almoſt all into water, while the cloſer and more tenacious craſſament, is in a manner deſtroyed. And in this caſe, the ſerum is almoſt as thin and fluxile as common water. But in very hot conſtitutions, or high inflammatory fevers, the craſſament, or globular part, is very much attenuated and fluxilized, and mixed ſo cloſely and intimately with the ſerum, that it will hardly ſeparate, or ſubſide, when taken out of the body, and left to ſettle in a cold place. And in the mean while,

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the ferum, or water of the blood, is extremely
thickened, coagulated, and rendered more
vifcid and tenacious, fo as to contract a ftrong
pellicle, or coat, almoft as tough as a real
fkin. The fame thing happens, when any
quantity of blood is let out into a bafon, or
porringer. The craffament by a greater de-
gree of cold, is condenfed, and falls to the
bottom in a ftrong coagulum, while the ferum
is left on the top, thin and limpid, like com-
mon water. And from hence it is evident,
that cold liquifies and fluxilizes the ferum, and
coagulates the craffament: and, on the contrary,
heat diffolves the craffament, while it coagu-
lates the ferum, and makes it more tenacious
and vifcid. Therefore, neither of thefe ef-
fects, with refpect to heat and cold, can be
affirmed of the compound mafs, or blood in
general.

2. Says the Doctor, *dryneſs ſtrengthens,
and watery moiſture weakens animal fibres;
for the very ſame reaſon that cold ſtrength-
ens, and heat weakens them.* I hope he
means all this within due bounds. For other-
wife, heat or cold, dryneſs or moiſture, may
ftrengthen or weaken animal fibres, and either
pomote or deſtroy their muſcular force and
action, as one or the other may happen to be
defective or redundant. But the Doctor is
ftill for laying down general rules, under a

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pompous appearance of demonstration, where nature has made no such limitations.

But the Doctor farther proves, that *cold strengthens or increases, and heat lessens the tenacity of the blood thus. For cold condenses, and heat rarifies the blood:* " But when the blood " is condensed, its particles are brought nearer " together, and when it is rarified, they are " removed farther asunder ; and when the " particles of the blood are brought nearer " together, their attractive forces are increased, " and when they are removed farther asunder, " those forces are lessened, all attractive forces " being stronger at less distances than at " greater; and when the attractive forces of " the particles of the blood are increased or " lessened, the cohesion of those particles, " which measures the tenacity of the blood, " is increased or lessened ; and therefore cold " increases, and heat lessens the tenacity of " the blood." This is the Doctor's reasoning in which, as any one must see, he supposes the tenacity and tenuity, or fluxility of the blood, to depend on its density, as it may happen occasionally, to be more or less condensed or rarified. He imagines, that the nearer the particles of any fluid are together, the more tenacious they must be, and the stronger their cohesion ; and the farther the centers of the particles are apart, the less is their cohesion, and the greater
their

their fluxility. But nothing can be more contrary to fact and experience. Oil is more viscid and tenacious than common water, tho' it is specifically lighter, and consequently its particles farther apart. Quickfilver is more fluxile, or less tenacious than common water, and may be divided into smaller parts, or a finer effluvium, tho' it is almost 14 times denser and heavier. The blood when most heated and rarified in fevers and inflammations, is more viscid and tenacious, than when it is most cooled and condensed in low languid cases; and when it comes nearest to the nature, and fluxility of common water. The Doctor therefore has quite mistaken the principle of cohesion, in the parts of bodies, which does not depend at all on the distances of their centres, but on their *quantity of contact*. If the particles were perfectly spherical, and hard or unyielding, they could touch but in a single point, and consequently, would not cohere at all. But when they touch in surfaces, tho' ever so small, there must be some cohesion, and this cohesion will be more or less in proportion to the quantity of contact. And this quantity of contact, or cohering force, is always in the ratio of the touching surfaces, and the density of the matter, or the number of particles which come into contact under the same surface, *i. e.* If the densities are equal, the cohesion will be as the surfaces, but if the surfaces

faces of contact are equal, the cohesion will be as the densities; and therefore universally in a ratio compounded of both. But as this gentleman every where assumes things as given, that are not, and cannot be given, and very often takes up with principles that are merely fictitious and groundless. It would be troublesome, as well as needless, to follow him farther. The whole debate betwixt the Doctor and me, now lies before you, Sir; and the Public, I believe, will expect your decision with some impatience.

I am S I R,

July 5, 1738.

Your most obedient,

Humble servant,

T. MORGAN.